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Delegation and Value Creation

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Delegation and Value Creation

Abstract:

Many scholars argue that the delegation of decision rights to independent institutions promotes trust and specific investments. We test this conjecture with variations of the trust game in which the back transfer decision is delegated to a third party. A randomly chosen third party with a fixed payment induces larger investments over time although the experimental design rules out reputation building. Changes in the third party's selection procedure eliminate this benefit. If the third party gets a reward for the appointment, delegation actually destroys trust. Investors (unwarrantedly) fear a diffusion of responsibility and lower back transfers in this case.

I. Introduction

Powerful people like to control economic rents. They are reluctant to delegate the distributions of these rents to another person who has different preferences and incentives. The benefits of delegation become even more questionable if the delegate does not have access to more relevant information, superior skills or other relevant resources. Nevertheless, a prominent literature suggests that delegation can provide tangible benefits to rent-seekers even when the delegate lacks superior capabilities, precisely because the incentives do not coincide. Rajan and Zingales (1998) and Blair and Stout (1999) argue that delegating ownership rights to an independent third party is beneficial for shareholders whenever a firm's competitive advantage depends on firm-specific investments from different stakeholders.[1] Through strategic delegation, investors relinquish the option of using the power of ownership *against each other*. Similarly Schelling (1960) discusses the use of third parties as a beneficial commitment device for a negotiating party in bargaining situations. We test these claims in this paper by studying how the delegation of distribution rights to a third party affects value creation and value appropriation.

It is a widely shared consensus that economies and firms attract more investments and generate more rents if independent institutions restrict powerful elites and guarantee property rights (Schelling, 1960, North, 1981, Knack and Keefer, 1997, Acemoglu, Johnson and Robinson, 2001).[2] While the suggested positive impact of independent third parties (or institutions) on investments is compelling, two problems restrict its empirical identification. First the requirements for a third party to be perceived as independent are unclear. Previous studies (e.g. Fershtman and Gneezy (2001), Hamman, Loewenstein and Weber (2010)) have shown that third party appointment and payment procedures can easily impair independence and may lead to diffusion of responsibility and biased actions. Powerful principals often try to compromise the third party's independence to distort the distribution of the rents in their favor. Second the empirical identification of the effect of independent

[1] Sengul, Gimeno and Dial (2012) support this argument in their review of the relevant management literature.

[2] The World Bank's annual "Doing Business" project reflects this consensus, as do corporate governance codes in the US, the UK, Germany and various other countries. Of course, the delegation of decision rights to independent institutions is only one of many potential strategies to encourage specific investments. Fairness concerns (Hackett (1994); Oosterbeek, Sonnemans and Van Velzen (2003), promises/threats (Ellingsen and Johannesson, 2004a, 2004b), sending simple text messages (Ellingsen and Johannesson, 2004a, 2004b), and shared ownership rights (Fehr, Kremhelmer and Schmidt, 2008), for example, may also cause individuals to make specific investments even in the absence of known reputations and repeated interactions.

institutions on investments is complicated by endogeneity concerns. As institutions and investments are jointly determined, correlations are likely to be confounded by omitted variables.

To address these problems we document results from an experiment in which investors are randomly assigned to different treatments. The standard investment or trust game established by Berg, Dickhaut and McCabe (1995), in which a sender transfers money to a receiver who gets the tripled transfer and decides how much of this money to return to the sender, serves as baseline treatment. In the delegation treatments a third party, instead of the receiver, decides on the back transfer to the sender. We manipulate the third party's incentives by using different selection and payment schemes and compare investments, back transfers and the net payoffs of both the sender and the receiver across treatments. Using a repeated stranger-matching procedure between senders and receivers we also study the development of these measures over time across the treatments.

This paper builds on and advances the literature on strategic delegation. Vickers (1985), Fershtman and Judd (1987), and Sklivas (1987) provide theoretical models of strategic delegation in oligopolistic markets. They show that firm owners obtain higher profits by delegating ownership rights to managers who do not behave as profit maximizers. In a Cournot-quantity competition, for example, firm owners can credibly commit to an aggressive strategic action by delegating ownership rights to managers whose rewards and preferences are linked to revenue growth. This causes rivals to reduce their output or to refrain from entering the market, which increases the focal firm's market share and profit.[3]

Fershtman and Gneezy (2001) show that the proposer's payoff in an ultimatum game is higher if she uses a third party who can be incentivized to make unfair offers. This happens because people are reluctant to reject (unfair) offers when both the proposer and the third party suffer a loss. Hamman et al. (2010) find that recipients receive significantly less money in a dictator game if principals hire third parties to act on their behalf. Diffusion of responsibility explains this finding: the principals feel less responsible for the outcome when hiring third parties and third parties feel that they are just following orders. Coffman (2011) and Bartling and Fischbacher (2012) show that delegation is beneficial to principals in allowing them to shift the responsibility for unfair allocations. They conducted dictator

[3] Huck, Konrad, Müller and Normann (2007) provide experimental evidence on strategic delegation in oligopolistic markets.

games with a delegation and punishment option and find that selfish principals receive less punishment if a third party implements an unfair allocation on their behalf.

While the abovementioned papers focused on the effect of strategic delegation on *value appropriation*, Fershtman (2007) and Charness et al. (2012) examine how strategic delegation affects *value creation*. Fershtman (2007) studies the impact of independent third parties in a one-shot investment game. Interestingly he finds that senders do not invest more if randomly selected third parties with a fixed payment decide on behalf of the receivers how much to return to the senders. Charness et al. (2012) find significantly higher effort levels in a gift exchange experiment if the wage decision is delegated to the workers who can choose their own wage. As the positive delegation effect on effort levels persists even when delegation is not deliberately chosen but randomly assigned, the authors explain their finding not by positive reciprocity but by a responsibility effect: the greater the perceived sense of responsibility, the more individuals engage in pro-social behavior.

We find significantly higher investments, on average, if the back transfer decision is delegated to a third party that is randomly assigned and receives a fixed payment. This increase develops over time even though our repeated stranger matching protocol excludes reputation building between investor and receiver. Initially the investment levels do not differ from the investment levels in the baseline treatment without delegation but they are significantly higher in later rounds.

We also find that selection and payment procedures can easily impair perceived independence and destroy trust. If the receiver is able to select a third party based on non-binding promises about the relative rent allocation and previous back transfer decisions and if the third party is paid its fixed payment only when it is selected, investments significantly decrease. Senders fear a diffusion of responsibility and lower back transfers, which actually does not occur. Third parties who materially benefit from appointments do not return less money than the receivers would return if they decided themselves.

Last not least we cannot substantiate the claim by Schelling (1960) and other studies (e.g. Sengul et al., 2012) that the original decision makers benefit from delegation to an independent third party. Thus, while the introduction of truly external third parties increases aggregate welfare and value creation, decision makers still have incentives to oppose such a policy.

The remainder of this paper is structured as follows: The next section explains our experimental design. Section III presents the behavioral predictions and Section IV the results. Section V concludes.

II. Experimental Design

This paper tests the effects of delegating the distribution rights on value creation and value appropriation by playing four variants of the "investment game" (or trust game) of Berg et al. (1995). At the beginning of the experiment subjects learned whether they were senders or receivers. They kept their role throughout the entire experiment. In the standard investment game – our baseline treatment *BASE* – one sender and one receiver were anonymously paired in each of 10 rounds according to a stranger matching protocol. At the beginning of the round both players were endowed with 10 Euros. The sender was asked to transfer a portion I of the endowment ($0 \leq I \leq 10$) to the receiver. This transfer measures the sender's investment. The experimenter tripled the transferred money so that $3I$ was passed to the receiver. Then, the receiver could pass any portion T of the money received ($0 \leq T \leq 3I$) back to the sender.

We implemented three variants of the investment game in which the back transfer decision was delegated to a third party. As in the standard investment game, the sender transferred between 0 and 10 Euros to the receiver and the receiver obtained three times the transferred money. Then, the third party decided what portion of the tripled investment was returned to the sender.

We exogenously manipulated the third party's selection and payment procedures across the different treatments. In the *random third party* treatment *RTP* the computer randomly assigned a third party in each round. All third parties received a fixed payment of 10 Euros regardless of the amount of money they transferred back to the sender. The *RTP* treatment describes a situation in which the third party is completely independent from the receiver.

In the *selected third party* treatment *STP* each receiver could choose a third party out of the potential third parties. At the beginning of each round the potential third parties made non-binding promises regarding the relative allocation of the tripled investment. Each third party had her own constant numeric identifier (1-9) across all rounds. As in the previous treatment, a third party received 10 Euros independent of any decision. While the third parties are materially independent in this treatment, the selection procedure can induce a bias towards the receiver.

In the *competitive third party* treatment *CTP* the third party selection mechanism was the same as in the *STP* treatment. However, the payment to a third party increased with the number of receivers selecting that third party: a third party received 5 Euros plus an additional 5 Euros for each appointment as third party. For example, a third party chosen by three receivers was paid 20 Euros (5 Euros fixed payment + 3*5 Euros per appointment). This payment scheme ensured the same average payment for third parties across all treatments. In this treatment both the payment and the selection procedure may compromise the third [illegible]

The third party's payment is paid by the experimenter in all three third party treatments to allow a simple comparison between the different treatments (see also Fershtman and Gneezy (2001)). Otherwise the introduction of a third party would reduce the pie to be divided between the sender and the receiver independent of the sender's investment.

As mentioned above we implemented a repeated stranger matching protocol for senders and receivers over 10 rounds in all treatments. The computer randomly matched senders and receivers in each round.[4] Senders invested without knowing which receiver and/or third party was selected or assigned in that round. Within groups full feedback was given at the end of each round. All details of the game such as the matching protocol, the payment schemes and the feedback rules were common knowledge. Table 1 summarizes the experimental design.

[4] Because we played 10 rounds but had only nine senders, receivers and third parties per session in *RTP*, *STP* and *CTP*, a perfect stranger matching protocol was not feasible. However, due to the sender's lack of knowledge of the assigned receiver's or third party's identity, the large number of subjects, and the random matching protocol, repeated game effects should not play a role.

TABLE 1
The experimental design (1 out of 10 rounds)

<table>
<tr><th></th><th>Baseline (BASE)</th><th>Random third party (RTP)</th><th>Selected third party (STP)</th><th>Competitive third party (CTP)</th></tr>
<tr><td>Stage 1</td><td colspan="4">Random anonymous matching of sender and receiver</td></tr>
<tr><td rowspan="2">Stage 2</td><td colspan="4">Sender makes investment I, receiver obtains 3* I</td></tr>
<tr><td>No third party</td><td>Random assignment of third party</td><td colspan="2">Potential third partys announce relative allocation (non-binding), receiver selects a third party</td></tr>
<tr><td>Stage 3</td><td>Receiver decides on the back transfer (T) to the sender</td><td colspan="3">Third party decides on the back transfer (T) to the sender</td></tr>
<tr><td rowspan="2">Stage 4</td><td colspan="4">Full information about decisions and payof fs within groups
Payoff sender: 10 - I + T
Payoff receiver: 10 + 3* I - T</td></tr>
<tr><td>No third party</td><td colspan="2">Payoff third party: 10</td><td>Payoff third party: 5+#selections*5</td></tr>
</table>

We conducted 14 sessions with a total of 373 subjects. All sessions took place in November and December 2012 at the *Lakelab* at the University of Konstanz. Subjects were students from the University of Konstanz who were recruited with the software "ORSEE" (Greiner, 2004). The experiments were computerized with the software "z-Tree" (Fischbacher, 2007). Each subject participated in one of the treatments only. Subjects were randomly assigned a role as sender, receiver, or third party upon arrival at the laboratory. Subjects kept that role during the entire experiment (no role reversal). They received written instructions and comprehension questions that had to be answered correctly before the experiment could start. An English translation of the instructions is included in the Appendix of this paper.[5] The sessions lasted approximately 50 minutes and subjects earned 13.65 Euros, on average.[6] To avoid wealth effects, one round was randomly selected to count for payment at the end of the experiment. All subjects received their payment privately.

III. Behavioral Predictions

In this section we describe the predictions concerning the behavior of the subjects in our experiment. Our main interest is in the investment decisions of the senders but these

[5] The experiments were conducted in German. The instructions in the Appendix provide a translation of the original instructions.
[6] At the time of the experiment, 1 Euro bought about 1.3 US-Dollars.

depend on the beliefs about the back transfers in the different treatments. Hence we focus on these back transfers initially.

The baseline treatment and the *RTP* treatment with randomly selected third parties provide two benchmarks. In the standard trust game a rational selfish receiver should not make any back transfer to the sender. However, inequity aversion, reciprocity or social norms induce some receivers to return money even at their own cost to implement the distribution they consider as appropriate. This effect is well documented in the literature.[7] We therefore expect at least some receivers to return money to the senders in case of a positive investment. In the *RTP* treatment the third party does not gain any material benefit from favoring the receiver over the sender (or vice versa). Hence, we expect higher back transfers in the *RTP* treatment than in the *BASE* treatment.

Hypothesis 1: The expected back transfers T for a given investment level I are ordered as follows across the *BASE* and *RTP* treatments: $0 < E(T_{BASE}|I) < E(T_{RTP}|I)$

In both the *STP* and *CTP* treatments third parties make non-binding promises on the relative rent allocation, but the number of selections affects the third party's payment only in the *CTP* treatment. In the *CTP* treatment third parties have an incentive to make a promise that reflects the presumed preferences of receivers.[8] In the first round these promises are the only meaningful criterion to distinguish the third parties. In rounds 2 to 10, receivers can select a third party based on actual decisions in previous rounds. By keeping their promises, third parties can build up a good reputation, which increases the likelihood of future selections by the same receiver. Therefore, the back transfers of the selected third parties in the *CTP* treatment should reflect the preferences of the receivers.

Hypothesis 2a: The back transfers T for a given investment level I are ordered as follows across the *BASE* and *CTP* treatments: $0 < E(T_{CTP}|I) = E(T_{BASE}|I)$

[7] Fehr (2009) provides a summary of the relevant results in this context, Johnson and Mislin (2011) a meta-analysis.

[8] Due to the heterogeneity of preferences among the different receivers we actually obtain multiple equilibria regarding the optimal proposal of a third party in this case. We assume that the proposing third parties can overcome the resulting coordination problem.

Our argument leading to Hypothesis 2a does not discuss empirical observations on delegation and the diffusion of responsibility. Fershtman and Gneezy (2001) and Hamman et al. (2010) find that delegation increases selfishness in ultimatum and dictator games if the agent is selected by the principal and incentivized to act in favor of the principal. Hamman et al. (2010, p. 1826) conclude that "[t]hrough the use of agents, […] accountability for morally questionable behavior can become vertically diffused". By selecting a delegate, principals simply hire (and implicitly pay) the most appropriate agent to act on their behalf while the agent feels that she is just carrying out orders. The agent is thus less reluctant to make selfish decisions on the principal's behalf than the principal herself would do. The effect of such diffusion of responsibility is equivalent to the effect of less intense fairness preferences in the *CTP* treatment than in the *BASE* treatment, which leads to the following competing hypothesis:

Hypothesis 2b: Assuming a diffusion of responsibility, the back transfers T for a given investment level I are ordered as follows across the *BASE* and *CTP* treatments: $0 \leq E(T_{CTP}|I) < E(T_{BASE}|I)$

Unlike in the *CTP* treatment, the third parties in the *STP* treatment have no financial incentives to match the preferences of the receivers in their promises, to keep that promise or to favor the receiver over the sender when deciding on the back transfers. Thus, we predict higher back transfers in the *STP* treatment than in the *CTP* treatment.

Hypothesis 3: The back transfers T for a given investment level I are ordered as follows across the *STP* and *CTP* treatments: $0 < E(T_{CTP}|I) < E(T_{STP}|I)$

Neglecting possible diffusion of responsibility, the combination of hypotheses 2a and 3 implies larger back transfers in the *STP* than in the *BASE* treatment. In the presence of a diffusion of responsibility, we cannot make a clear-cut prediction whether back transfers in the *STP* treatment are higher than, lower than or equal to the back transfers in the *BASE* treatment. In the *BASE* treatment the receiver may choose an uneven distribution because she has monetary incentives for value appropriation whereas in the *STP* treatment the third party may choose an uneven distribution because she does not feel responsible for implementing a fair allocation.

Hypothesis 4a: Neglecting possible diffusion of responsibility, the back transfers T for a given investment level I are ordered as follows across the *BASE* and *STP* treatments: $0 < E(T_{BASE}|I) < E(T_{STP}|I)$

Hypothesis 4b: Assuming a diffusion of responsibility, we do not expect a systematic treatment difference in the back transfers T for a given investment level I in the *BASE* and *STP* treatments: $0 < \left(E(T_{BASE}|I) \lesseqgtr E(T_{STP}|I)\right)$

In monetary terms the incentives of third parties in the *STP* and *RTP* treatments do not differ. Yet there are some non-monetary motives why the rewards in the *STP* treatment will be smaller than in the treatment with randomly selected third parties (*RTP*). Several studies have shown that many individuals perceive lying as a violation of social norms that induces psychological costs to themselves and others (Brandts and Charness, 2003, Croson, Boles and Murnighan, 2003, Sánchez-Pagés and Vorsatz, 2007, 2009). Thus, third parties in the *STP* treatment tend to stick to their promised distributions. If third parties have heterogeneous fairness preferences and make different promises, the honesty motive allows receivers to select third parties according to their promises. As a result the selected third parties will be somewhat biased towards the receivers. Moreover some third parties might get ego-rents just from being selected (Rogoff, 1990, Garrì, 2010, Fehr, Herz and Wilkening, forthcoming). These people then also benefit from building up a reputation by implementing their promises.

Hypothesis 5: The expected back transfers T for a given investment level I are ordered as follows across the *STP* and *RTP* treatments: $0 < E(T_{STP}|I) < E(T_{RTP}|I)$

Depending on the diffusion of responsibility effect, hypotheses 1 to 5 lead to the following relationship between expected back transfers T for a given investment level I across all treatments:

$$\text{Back transfers without diffusion: } 0 < E(T_{CTP}|I) = E(T_{BASE}|I) < E(T_{STP}|I) < E(T_{RTP}|I),$$

$$\text{Back transfers with diffusion: } 0 \leq E(T_{CTP}|I) < \left(E(T_{BASE}|I) \lesseqgtr E(T_{STP}|I)\right) < E(T_{RTP}|I).$$

A sender will invest as long as the back transfer is at least as large as the initial investment. Thus, the higher the expected back transfer in a treatment, the more likely this condition will be met and the higher the sender's expected investment will be. Thus we obtain the following two competing hypotheses, depending on the diffusion of responsibility:

Hypothesis 6a: Neglecting a diffusion of responsibility between receivers and selected third parties, the expected investment levels I are ordered as follows across the treatments:

$$0 < E(I_{CTP}) = E(I_{BASE}) < E(I_{STP}) < E(I_{RTP})$$

Hypothesis 6b: Assuming a diffusion of responsibility between receivers and selected third parties, the expected investment levels I are ordered as follows across the treatments:

$$0 \leq E(I_{CTP}) < \left(E(I_{BASE}) \lesseqgtr E(I_{STP})\right) < E(I_{RTP})$$

IV. Experimental Results

We discuss the experimental results as follows. The first two subsections test the hypotheses by comparing aggregate treatment outcomes. In subsection A we analyze investments and in subsection B back transfer decisions. In subsection C we look at the evolution of investments and back transfer decisions over time. Subsection D presents an analysis of the sender's and receiver's payoffs. Finally in subsection E we analyze third party selections in the treatments *STP* and *CTP* to investigate any diffusion of responsibility between the receiver and the selected third party in more detail.

Table 2 displays a summary of the average investment by the sender, the average proportion returned by the receiver or the third party, and the sender's and receiver's payoffs. The average payoff of the third party was 10 in all third party treatments.

TABLE 2

Summary of behavior

	BASE		*RTP*		*STP*		*CTP*	
	Mean	*Std. Dev.*	*Mean*	*Std. Dev.*	*Mean*	*Std. Dev.*	*Mean*	*Std. Dev.*
Investments	5.10	3.64	6.24	3.42	4.81	3.22	3.52	3.35
Proportion returned[a]	0.32	0.19	0.58	0.21	0.39	0.24	0.29	0.25
Sender's payoff	10.10	3.52	14.80	5.34	10.80	3.19	9.45	4.18
Receiver's payoff	20.11	7.84	17.68	6.05	18.83	7.77	17.59	7.46
Sessions	3		4		3		4	
Sender/receiver/third party	41/41/n.a.		35/35/35		27/27/27		35/35/35	
Rounds	10		10		10		10	
Observations	410/410/n.a.		350/350/350		270/270/270		350/350/350	

[a] To calculate the means and standard deviations of the proportions returned, we only used observations with an initial investment above 0. If the sender invested 0, the receiver/third party was not able to return anything. Number of observations are 325 in *BASE*, 314 in *RTP*, 216 in *STP* and 247 in *CTP*, respectively.

A. Investments

The descriptive statistics in Table 2 already suggest investment differences across treatments. T-tests and non-parametric Wilcoxon rank-sum tests (all two-tailed) of differences in mean investments across all rounds, using each investor as one observation, support this impression. They show significantly higher investments in *RTP* ($t = 1.73$, $p = 0.09$; $z = 1.73, p = 0.08$) and significantly lower investments in *CTP* ($t = -2.73$, $p < 0.01$; $z = -2.63, p < 0.01$) than in *BASE*. Investments in *STP* are not significantly different from baseline investments ($t = 0.45, p = 0.65; z = 0.40, p = 0.69$).

Table 3 presents OLS estimates of the treatment effects on investments and proportions returned. The main explanatory variables are dummies for each third party treatment (the baseline treatment serves as reference category). As individuals were randomly selected into treatments, treatment dummies are exogenous and OLS provides unbiased estimates. To allow for any arbitrary correlation of the error terms within a session, we use robust standard errors clustered at the session level.[9]

[9] The results remain virtually the same if the standard errors are clustered on the subject level or if we estimate random effects models.

TABLE 3

Third-party influence on investments and proportions returned

Dependent variable	Investments	Proportion returned	
Sample	All obs.	obs. with investment>0	
	(1)	(2)	(3)
BASE	Ref. group	Ref. group	Ref. group
RTP	1.135**	0.255***	0.253***
	(0.519)	(0.026)	(0.025)
STP	-0.288	0.066*	0.068*
	(0.545)	(0.036)	(0.036)
CTP	-1.585***	-0.038	-0.031
	(0.505)	(0.046)	(0.045)
Investments			0.005
			(0.003)
Constant	5.102***	0.325***	0.294***
	(0.476)	(0.022)	(0.034)
Number of observations	1380	1102	1102
Number of subjects	138	137	137
R^2	0.080	0.219	0.222

Notes: RTP, STP and *CTP* are treatment dummies. Table displays OLS coefficients with White robust standard errors clustered at the session level in parentheses. Significance levels are denoted by *** 1 percent, ** 5 percent, * 10 percent (two-tailed tests).

The OLS treatment effects on investments (Model 1) are in line with the results from the t-tests and the Wilcoxon rank-sum tests: investments were significantly higher in *RTP* ($p = 0.048$) and significantly lower in *CTP* ($p = 0.008$) than in the baseline.[10] Investments in *STP* were not significantly different from investments in *BASE* ($p = 0.606$). However, investments in *STP* were significantly higher than in *CTP* ($p = 0.001$).

RESULT 1: *Delegating the back transfer decision to a randomly assigned third party with a fixed salary significantly increases investments relative to the baseline case where the back transfer decision is not delegated.*

RESULT 2: *Delegating the back transfer decision to a selected third party decreases investments relative to the baseline case if the third party receives a salary that increases with the number of selections.*

[10] The inclusion of round fixed effects would not change our results in any significant way because they are uncorrelated with the treatment dummies.

The empirical analyses confirm hypothesis 6b (investments with an expected diffusion of responsibility) and reject the alternative hypothesis 6a (investments without an expected diffusion of responsibility). The lower investments in *CTP* than in *BASE* indicate that senders feared receiving lower back transfers from an incentivized third party than from the receiver. The fact that investments in *STP* and *BASE* do not significantly differ shows that senders perceive the receivers with financial incentives as equally trustworthy as the selected third parties without financial incentives.

B. Back transfers

Models 2 and 3 in Table 3 show the OLS results of the treatment effects on the proportion returned. In Model 3 we additionally control for investments because they differ across treatments. The receiver or the third party could only decide to return something if the sender had made a positive investment. In models 2 and 3 we therefore restrict the sample to observations with investments above 0.[11] Table 2 shows that a randomly selected third party returned almost twice as much as the receiver returned in the *BASE* treatment. The statistical inference in Table 3 documents that the difference is statistically significant, which supports hypothesis 1.[12]

RESULT 3: The return proportion is higher if a randomly assigned third party with a fixed payment decides on the back transfer in place of the receiver.

The return proportion in the *CTP* treatment was lower and the return proportion in the *STP* treatment higher than in the *BASE* treatment. Whereas the difference between the *CTP* and the *BASE* treatments is not statistically significant, the difference between the *STP* and the *BASE* treatments is statistically significant at the 10% level. Overall, this evidence confirms hypotheses 2a and 4a that back transfers do not decline because of a diffusion of responsibility. The results imply that the senders' expectations about diffused responsibility are actually too pessimistic.

[11] The exclusion of observations with 0 investments reduces the sample by 278 observations and by one subject who experienced 0 investments in all ten rounds.

[12] T-tests and non-parametric Wilcoxon rank-sum tests of differences in the mean return proportion across all rounds, using each decision maker (receiver in the *BASE* treatment and third party in the *RTP*, *STP* and *CTP* treatments) as one observation, also support these results.

RESULT 4: The introduction of third parties does not decrease back transfers.

In line with hypotheses 3 and 5, back transfers in the *CTP* treatment were significantly lower ($t = 2.24,\ p = 0.03;\ z = 2.21, p = 0.03$) and those in the *RTP* treatment were significantly higher ($t = 3.82,\ p < 0.001;\ z = 3.62, p < 0.001$) than in the *STP* treatment.[13]

C. Evolution of trust and investments

So far we have pooled all ten rounds and analyzed mean investments and return proportions. This section looks at temporal developments. Figure 1 illustrates the evolution of investments across rounds and treatments. Whereas investments generally decreased over time in the *BASE* treatment, investments tended to increase in the *RTP* treatment. In the treatments in which the third party was selected (*STP*, *CTP*) investments increased initially but generally decreased in later rounds.

[13] Again we used t-tests and non-parametric Wilcoxon rank-sum tests of differences in mean back transfers across all rounds, taking each decision maker as one observation. In the *BASE* treatment the decision maker is the receiver, whereas in the *RTP*, *STP*, and *CTP* the decision maker is the (selected) third party.

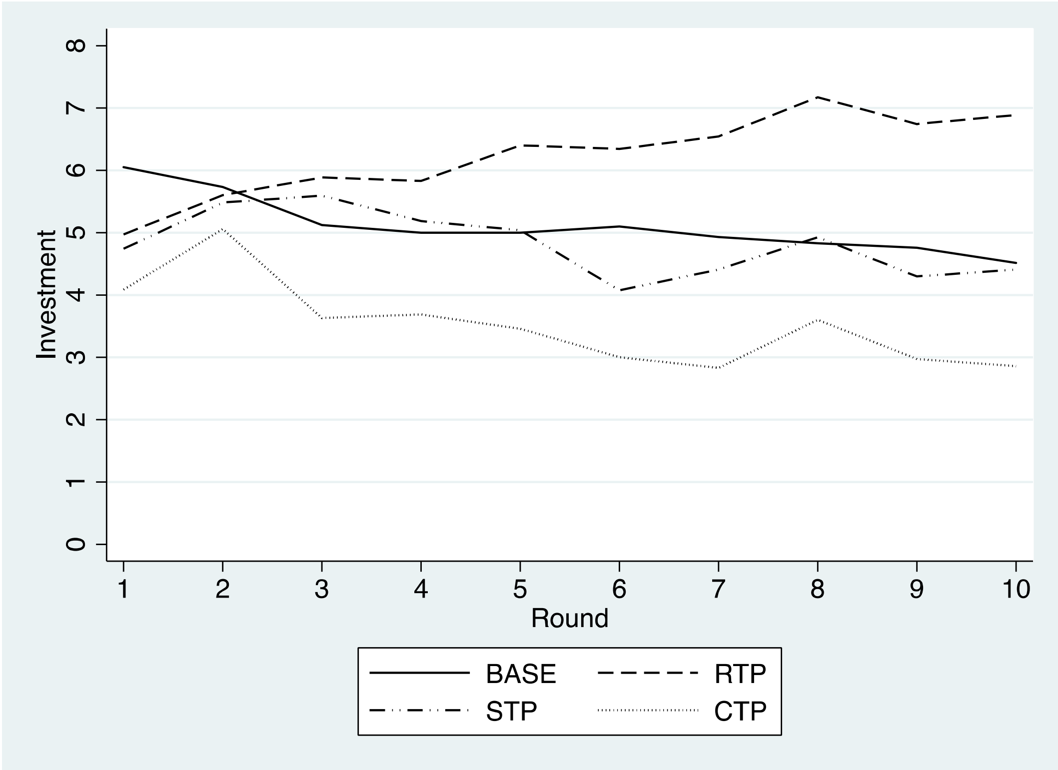

Figure 1: Mean investments by treatment and round

Table 4 shows the results of t-tests of differences in mean investments between treatments and baseline for each round.[14] In rounds 1-4, mean investment in the *RTP* treatment was not significantly higher than in the *BASE* treatment. Comparing mean investments in rounds 5 to 10, we observe much larger and statistically significant differences ($BASE$: 4.85; RTP: 6.68; $t = 2.54, p < 0.05$). In the *STP* treatment, investments were very similar to the investments in the *BASE* treatment. Except in round 1, the difference was never statistically significant. When comparing investments between the *BASE* and *CTP* treatments, investments were always lower in the *CTP* treatment and the difference was statistically significant in every round except rounds 2 and 8.

[14] The statistical tests are robust to the use of alternative tests like non-parametric Wilcoxon rank-sum tests.

TABLE 4
Mean investments by treatment and round

Round	*BASE* (n=41)	*RTP* (n=35)	t-statistics of difference with *BASE*	*STP* (n=27)	t-statistics of difference with *BASE*	*CTP* (n=35)	t-statistics of difference with *BASE*
1	6.05	4.97	1.43	4.74	1.67*	4.09	3.03***
2	5.73	5.60	0.40	5.48	0.22	5.06	0.83
3	5.12	5.89	0.93	5.59	-0.46	3.63	1.81*
4	5.00	5.83	0.97	5.19	-0.17	3.69	1.66*
5	5.00	6.40	1.61	5.04	0.04	3.46	1.86*
6	5.10	6.34	1.37	4.07	1.15	3.00	2.30**
7	4.93	6.54	1.83*	4.41	0.78	2.83	2.21**
8	4.83	7.17	2.58***	4.93	0.02	3.60	1.39
9	4.76	6.74	2.22**	4.30	0.49	2.97	2.15**
10	4.51	6.89	2.62***	4.41	0.07	2.86	1.88*

Notes: Significance levels (two-tailed) are denoted by *** 1 percent, ** 5 percent, * 10 percent. Non-parametric Wilcoxon-ranksum tests lead to virtually the same results.

Because senders and receivers were randomly matched in each round and because senders were not informed of the matched receiver's and/or the (selected) third party's identity before making the investment, reputation effects are ruled out. Nevertheless, as full feedback was given at the end of each round, senders could still learn about aggregate behavior over time. Learning includes both updating priors concerning the expected behaviors of the others and an improved understanding of the game (Muller, Sefton, Steinberg and Vesterlund, 2008). The following sections test whether learning could explain the treatment effects in later rounds.

Figure 2 shows the fraction of positive net returns per treatment and round. If more than one third of the tripled investment is returned, the sender obtains a positive net return on investment. Whereas the fraction of senders experiencing a positive net return was below 50 percent in the *BASE* treatment in each round, it was always substantially above 50 percent in the *RTP* treatment. In the *STP* and the *CTP* treatment most senders experienced negative net returns as they invested more than they got back. The fraction of positive net returns remained relatively stable over time.

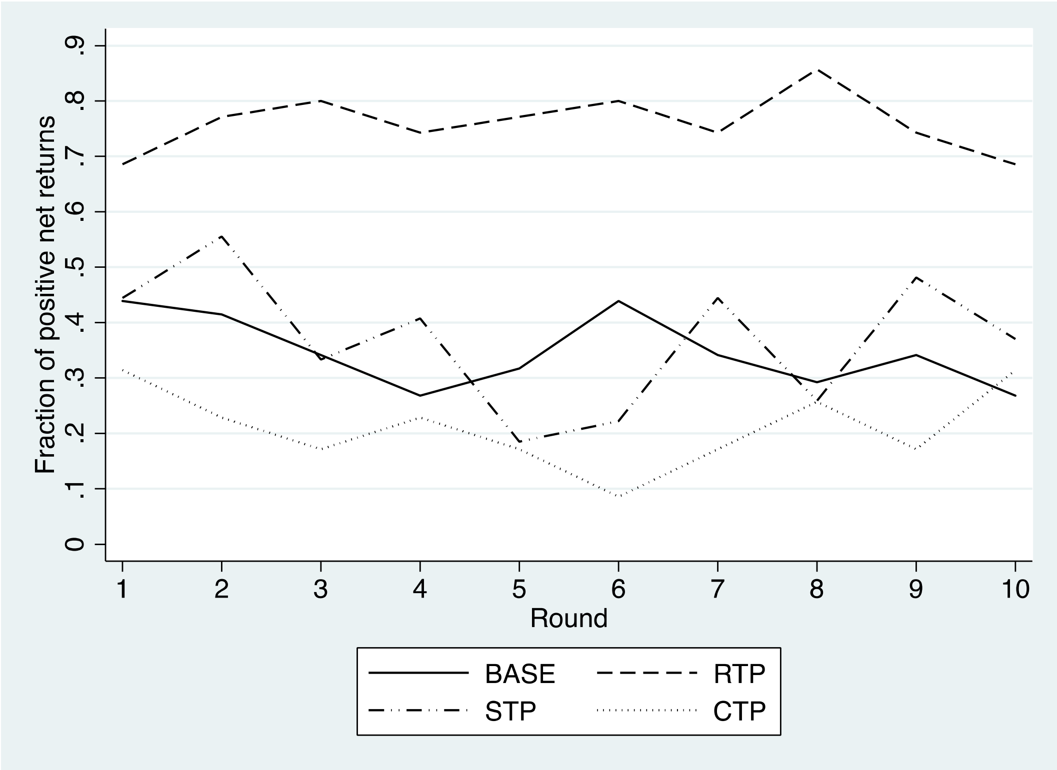

Figure 2: Fractions of positive net returns per round and treatment

Table 5 provides an econometric test of learning processes at the individual level. More specifically, we include the fraction of positive net returns a sender experienced in previous rounds as a control variable in our model. The variable has a mean of 0.44 and varies between 0 and 1. A value of 1 indicates that back transfers exceeded investments in all previous rounds. A value of 0 indicates that investments exceeded back transfers or that the sender invested nothing in all previous rounds. As learning is only possible from round 2 onwards, Table 5 excludes first round observations.

TABLE 5
Evolution of trust and investments

Dependent variable	Investment		
Sample	Rounds 2 to 10		
	(1)	(2)	(3)
BASE	Ref. group	Ref. group	Ref. group
RTP	1.380**	-0.164	-0.211
	(0.579)	(0.343)	(0.775)
STP	-0.174	-0.282	-0.127
	(0.566)	(0.510)	(0.733)
CTP	-1.543**	-0.910*	-0.420
	(0.552)	(0.456)	(0.348)
Fraction of positive net returns (up to t-1)		4.214***	4.766***
		(0.509)	(0.131)
Fraction of positive net returns * *RTP*			-0.207
			(1.254)
Fraction of positive net returns * *STP*			-0.416
			(1.083)
Fraction of positive net returns * *CTP*			-1.762**
			(0.669)
Constant	4.997***	3.390***	3.179
	(0.530)	(0.353)	(0.567)
# observations	1242	1242	1242
# subjects	138	138	138
R^2	0.082	0.207	0.211

Notes: RTP, STP and *CTP* are treatment dummies, *Fraction of positive net returns* is the share of previous periods in which the back transfer was higher than the investment. Table displays OLS coefficients with White robust standard errors clustered at the session level in parentheses. Significance levels are denoted by *** 1 percent, ** 5 percent, * 10 percent (two-tailed tests).

Model 1 in Table 5 is a replication of Model 1 in Table 3 except that the first round is not considered. When controlling for the *fraction of positive net returns* in previous rounds in Model 2, the positive *RTP* treatment effect becomes small and statistically insignificant. The size of the negative *CTP* treatment effect also decreases but the effect remains statistically significant at the 10 percent level. The new control variable *fraction of positive net returns* has a large and significantly positive influence on investments. Senders who had experienced only positive net returns in previous rounds invested 4.2 Euros more than senders who had experienced only negative net returns in previous rounds. Model 3 additionally includes interaction terms of the treatment dummies with the control variable *fraction of positive net returns.* Table 5 shows that the interaction effect of the fraction of positive net returns and *CTP* is significantly negative, which implies that investors in the *CTP* treatment react more cautiously to positive net returns in previous rounds.

RESULT 5: *Senders increase investments if they obtained positive net returns on their investments in previous rounds.*

Table 5 shows that third party independence is a not a sufficient condition to encourage investments. An increase in investments also requires repeated interactions under the same institution (but not necessarily the same person) and the experience of positive net returns on investment. Even with the stranger matching protocol, senders update their back transfer beliefs according to the net returns in previous rounds. As the return proportions and thus the fractions of senders experiencing a positive net return were relatively stable over time within a treatment (see Figure 2), investments increased over time in the *RTP* treatment in which back transfers were higher than investments on average and investments decreased over time in the other treatments in which back transfers were lower than investments on average.

D. Payoffs

Table 6 reports the results with the sender's and receiver's payoffs as dependent variables and treatment dummies as independent variables. For each dependent variable we also estimate a specification with initial investments as control variable because investments determine the created value that can be divided between the sender and the receiver.

TABLE 6

Third-party influence on payoffs

Dependent variable	Sender's payoff		Receiver's payoff	
	(1)	(2)	(3)	(4)
BASE	Ref. group	Ref. group	Ref. group	Ref. group
RTP	4.700***	4.455***	-2.430***	-4.455***
	(0.532)	(0.475)	(0.741)	(0.475)
STP	0.699	0.761	-1.274	-0.761
	(0.490)	(0.493)	(1.134)	(0.493)
CTP	-0.649	-0.307	-2.522***	0.307
	(0.447)	(0.433)	(0.790)	(0.433)
Investments		0.216*		1.784***
		(0.111)		(0.111)
Constant	10.098***	8.997***	20.107***	11.003***
	(0.284)	(0.556)	(0.673)	(0.556)
Number of observations	1380	1380	1380	1380
Number of subjects	138	138	138	138
R^2	0.209	0.235	0.021	0.702

Notes: RTP, STP and *CTP* are treatment dummies. Table displays OLS coefficients with White robust standard errors clustered at the session level in parentheses. Significance levels are denoted by *** 1 percent, ** 5 percent, * 10 percent (two-tailed tests).

The sender's payoff was significantly higher when the return decision was delegated to a randomly assigned third party with a fixed payment (*RTP*). While the payoff was slightly higher if the return decision was delegated to a selected third party with a fixed payment (*STP*) and slightly lower if the return decision was delegated to a third party whose payment increased with the number of selections (*CTP*), the latter two effects are not significantly different from 0. Model 2 shows that an additional point of investment increased the sender's payoff by 0.216.

RESULT 6: *The sender obtains a significantly higher payoff if the return decision is delegated to a randomly assigned third party with a fixed payment.*

Models 3 and 4 in Table 6 compare the receiver's payoff across treatments. Model 3 shows that the receiver's payoff was considerably lower in all three third party treatments. The payoff reduction in comparison to the *BASE* treatment was large and statistically significant in the treatments *RTP* and *CTP* and smaller and statistically insignificant in the treatment *STP*. The explanations for the lower payoffs differ, however. In the treatments *RTP* and *STP* the payoff was lower because third parties returned more money, whereas in the treatment *CTP* the payoff was lower due to lower investments. The *CTP* coefficient becomes very small and statistically insignificant when controlling for investments in Model 4.

RESULT 7: *The receiver obtains a lower payoff if the return decision is delegated to a third party, either due to higher relative back transfers or due to lower initial investments.*

Result 7 implies that receivers are unlikely to voluntarily delegate ownership rights to a third party. Whereas Coffman (2011) and Bartling and Fischbacher (2012) have shown that people can avoid being punished for unfair decisions when delegating this decision, we show that delegating reward decisions is not profitable.

E. Third party selection and diffusion of responsibility

We have learned that while senders expect a diffusion of responsibility, such behavior is not actually observed. In this subsection we analyze the reasons for the latter finding in more detail. More specifically, we analyze third party selection in the treatments *STP* and *CTP*.

We argued in our predictions that without a diffusion of responsibility receivers will select third parties who promise to return the same proportion as the receivers themselves return in the *BASE* treatment. However, if receivers feel less responsible and have less intense fairness concerns when delegating the return decision to third parties, receivers will select third parties who promise to return less to the senders on average than the receivers themselves return in the *BASE* treatment.

TABLE 7

Third-party selection, promises and decisions

	BASE		*STP*		*CTP*	
	Mean	*Std. Dev.*	*Mean*	*Std. Dev.*	*Mean*	*Std. Dev.*
Proportion returned[a]	0.32	0.19	0.39	0.24	0.29	0.25
Promised return proportion of *selected* third parties			0.25	0.24	0.17	0.18
Promised return proportion of *all* third parties			0.36	0.28	0.21	0.22

[a] To calculate the means and standard deviations of the proportions returned, we only used observations with an initial investment above 0. If the sender invested 0, the receiver/third party was not able to return anything. Number of observations are 325 in *BASE,* 314 in *RTP,* 216 in *STP* and 247 in *CTP,* respectively.

Table 7 illustrates the means and the standard deviations of the actual proportion returned, the promised return proportion of the *selected* third parties and the promised return proportion of *all* third parties per treatment. Table 7 shows that receivers in both the

STP and *CTP* treatments selected third parties who promised to return less than the receivers returned themselves in the *BASE* treatment. Both differences are highly statistically significant when conducting T-tests and non-parametric Wilcoxon rank-sum tests ($BASE\ and\ STP$: $t = 4.38, p < 0.001$; $z = 4.42, p < 0.001$; $BASE\ and\ CTP$: $t = 9.62$, $p < 0.001$; $z = 8.94$, $p < 0.001$). Thus, receivers are less reluctant to act more selfishly if third parties implement the decisions on their behalf, which indicates a diffusion of responsibility. The fact that the average promised return proportion of the *selected* third parties was significantly lower (STP: $t = 4.57, p < 0.001, z = 4.18, p < 0.001$; CTP: $t = 2.86, p < 0.01, z = 2.55, p = 0.011$) than the average promised return proportion of *all* third parties shows that third parties with lower promised return proportions were more likely to be selected.

The correlations between the promised and the actual back transfers are 0.48 and 0.52 in the *STP* and *CTP* treatments, respectively,[15] which indicates that some third parties did not keep their promises, namely 57 percent in the *STP* treatment and 42 percent in the *CTP* treatment. Those third parties who did not keep their promises tended to return significantly more than promised (STP: $t = 6.42, p < 0.001, z = 6.38, p < 0.001$; CTP: $t = 5.87, p < 0.001, z = 5.22, p < 0.001$). This explains why the actual return proportions in the *CTP* treatment were not significantly lower and the return proportions in the *STP* treatment even higher than in the *BASE* treatment.

V. Conclusion

The delegation of ownership and decision rights to an independent third party has been a prominent suggestion to promote trust and specific investments both at the state and the firm level (Schelling, 1960, Rajan and Zingales, 1998, Acemoglu et al., 2001) but robust empirical evidence has been missing so far. The experimental evidence presented in this paper reveals that the selection and payment procedure of the third party strongly moderates the effect on investments. Compared to a standard investment game without delegation (Berg et al., 1995), average investments were significantly higher if a randomly assigned third party with a fixed payment, rather than the receiver, decided on the back

[15] Correlations do not include the cases in which investments were zero. Because third parties could only transfer a whole number of Euros we calculated the promised back transfers based on the promised return proportions and the investments and then rounded this value to the next feasible integer.

transfer. A more detailed analysis of investments revealed that the independent third parties induced higher investments only after a few rounds. Even though reputation building was ruled out due to the stranger matching protocol, senders had to experience positive returns on investments before they increased their investments. Controlling for the fraction of positive net returns in previous rounds largely eliminated the treatment effects.

Delegating the back transfer decision to a third party who benefits from being selected by the receiver significantly decreased investments. Investors feared that selected third parties may feel less responsible for the outcome and therefore implement lower back transfer than the receivers would implement if they decided themselves. However, while Hamman et al. (2010) found evidence for such diffusion of responsibility in dictator games, we did not observe that third parties behave more self-interestedly on the receivers' behalf than the receivers would have done if they had to decide themselves.

The management literature on strategic delegation typically assumes that delegating decision making leads to outcomes that are beneficial for the original decision maker (Sengul et al., 2012). Such a positive effect was confirmed in ultimatum games (Fershtman and Gneezy, 2001), and dictator games both with a punishment option (Coffman, 2011; Bartling and Fischbacher, 2012) and without a punishment option (Hamman et al., 2010). In investment games, however, we find that the receivers as original decision makers did not profit from delegation. The receivers obtained lower payoffs in all delegation treatments. Thus, kind actions, like returning money, should not be delegated.

Decision makers face the following trade-off: On the one hand delegating ownership rights to an absolutely independent third party increases value creation, on the other hand it decreases value appropriation. Delegating ownership rights to a selected third party with financial incentives to match the original decision makers' preferences increases value appropriation, but decreases value creation. Our results suggest that the negative effect dominates in both cases, which may explain why decision makers are rather reluctant to delegate ownership rights and install independent institutions on their own (Acemoglu and Robinson, 2006).

Our results highlight the importance of the specific design of appointment and payment procedures of independent institutions. Our insights benefit from the privilege of experimental studies in creating counterfactual environments. In particular we were capable of studying a relevant benchmark case, the effect of truly exogenously appointed third parties with no financial stakes in the distribution decisions. Such third parties induced the most efficient investment behavior. In business, random selection has been less accepted so far. As delegating ownership rights to a completely independent third party decreases the payoff of the delegating person, the creation of independent institutions is unlikely to happen on its own. In public policy making that is more focused on aggregate welfare than individual payoffs, random third party assignment may be more feasible. In law, for example, random third party assignment is commonly implemented for trial juries.

In representative democracies elected politicians hold the ultimate power in between the elections. In firms the owners hold the ultimate decision rights when contracts or regulations do not provide specific restrictions. Nevertheless, these politicians and owners often call for third parties in cases of controversial decisions. The results of our paper question the prudence of such a delegation. They show that third party selection creates inefficiencies because first, any appointment committee could select people who are ready to return a favor, and second, even if this was not the case, outside investors might consider the selected third party to be biased against them. In such cases, reputation-building activities like fostering repeated interactions between the same partners and/or informing investors about past behavior (Berg et al., 1995, Bohnet and Huck, 2004) could serve as alternative strategies to increase specific investments.

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Appendix A: Instructions

General instructions for the participants

(Note: This is an English translation of the German instructions of the baseline treatment BASE. We integrated control questions about the experiment into the z-tree file.)

We would like to welcome you to this economic experiment.

Your decisions and if applicable the decisions of the other participants in this experiment can influence your payment. It is important that you carefully read these instructions. If you have any questions, please ask **before** the experiment starts. All participants receive the same instructions.

During the experiment it is not allowed to talk with other participants. Disregard of this rule will lead to exclusion from the experiment and the payment.

During the experiment we do not talk about Euros. We talk about points instead. Your payment will be first calculated in points. The total number of points you will achieve in this experiment will be converted into Euros at the end with a conversion rate of:

1 point = 1 Euro

We will pay out the payment in cash at the end of today's experiment. On the following pages we explain the detailed procedure of this experiment.

Structure of the experiment

In this experiment you are always a group of two. In this pairing there is always a **participant A** and a **participant B**. At the beginning of the experiment the computer randomly determines if you are a participant A or B. You will keep the same role during the whole experiment.

The experiment lasts for ten rounds. **In each round a new pairing will be formed at random.** We explain the procedure of one round. All ten rounds have the same procedure. You will be paid according to the points achieved in a randomly chosen round.

Participant A and participant B are endowed with 10 points. Participant A can send between 0 and 10 points to participant B. The amount sent is tripled by the experimenter and given to B. Participant B can now decide how many of the received points to return back to participant A. This back transfer is not tripled.

The participants will receive the following payment, if the computer determines this round for the payment:

- Participant A: 10 points – amount sent by participant A + back transfer
- Participant B: 10 points + 3 × (amount sent by participant A) – back transfer

At the end of a round the participants will be informed about their points earned in that round.

Sequence of decisions:

A round proceeds on the screen as follows. Firstly, participant A decides on the transfer to B by entering a number between 0 and 10 and reports his belief about the expected back transfer. Parallel participant B reports his belief concerning the amount sent by participant A.

Participant B then learns how many points A has sent and how many points B accordingly has received. Then participant B decides on the back transfer by entering the corresponding amount.

General instructions for the participants

(Note: This is an English translation of the German instructions of the treatment RTP. We integrated control questions about the experiment into the z-tree file.)

Structure of the experiment

In this experiment you are always a group of three. In this triad there is always a **participant A**, a **participant B** and a **participant C**. At the beginning of the experiment the computer randomly determines if you are a participant A, B or C. You will keep the same role during the whole experiment.

The experiment lasts for ten rounds. **In each round the triad will be newly formed at random**. We explain the procedure of one round. All ten rounds have the same procedure. You will be paid according to the points achieved in a randomly chosen round.

Participant A and participant B are endowed with 10 points. Participant A can send between 0 and 10 points to participant B. The amount sent is tripled by the experimenter and given to B. Participant C can now decide how many of the received points to return back to participant A. This back transfer is not tripled. C cannot return more than B received from A. The 10 points that B received from the experimenter remain with B in any case. **Participant C receives 10 points from the experimenter independent of her decision.**

The participants will receive the following payment, if the computer determines this round for the payment:

- Participant A: 10 points – amount sent by participant A + back transfer
- Participant B: 10 points + 3 × (amount sent by participant A) – back transfer
- Participant C: 10 points

At the end of a round the participants will be informed about their points earned in that round.

Sequence of decisions:

A round proceeds on the screen as follows. Firstly, participant A decides on the transfer to B by entering a number between 0 and 10 and reports his belief about the expected back transfer. Parallel participant B reports his belief concerning the amount sent by participant A.

Participant C then learns how many points A has sent and how many points B accordingly has received. Then C decides on the back transfer by entering the corresponding amount. Parallel participant B reports his belief concerning the amount sent by participant C.

General instructions for the participants

(Note: This is an English translation of the German instructions of treatment STP. We integrated control questions about the experiment into the z-tree file.)

Structure of the experiment

In this experiment you are always a group of three. In this triad there is always a participant A, a participant B and a participant C. At the beginning of the experiment the computer randomly determines if you are a participant A, B or C. You will keep the same role during the whole experiment.

The experiment lasts for ten rounds. We explain the procedure of one round. All ten rounds have the same procedure. You will be paid according to the points achieved in a randomly chosen round.

Participant A and participant B are endowed with 10 points. Participant A can send between 0 and 10 points to participant B. The amount sent is tripled by the experimenter and given to B. A **from B selected participant C** can now decide how many of the received points to return back to participant A. This back transfer is not tripled. C cannot return more than B received from A. The 10 points that B received from the experimenter remain with B in any case.

Participants A and B are randomly matched in each round. Participant B selects a participant C in each round. The selection procedure runs as follows: All participants C tell the participants B, what **percentage** of the transferred money they want to remain with B. At this stage each participant C gets a number, which clearly identifies her over all rounds, without compromising her anonymity. Afterwards each participant B selects a participant C. Note that several participants B can simultaneously select a player C. The announcement of participant C concerning the back transfer is not binding. Thus, a selected participant C can reconsider her decision regarding the back transfer. **Participant C receives 10 points from the experimenter independent of her decision.**

The participants will receive the following payment, if the computer determines this round for the payment:

– Participant A: 10 points – amount sent by participant A + back transfer
– Participant B: 10 points + 3 × (amount sent by participant A) – back transfer
– Participant C: 10 points

At the end of a round the participants will be informed about their points earned in that round.

Sequence of decisions:

A round proceeds on the screen as follows. Firstly, all participants C inform the participants B about the percentage of the transferred money that should remain with B. Secondly, each participant B selects a participant C. Simultaneously participant A decides on the transfer to B by entering a number between 0 and 10 and reports his belief concerning the expected back transfer.

The selected participant C then learns how many points A has sent and how many points B accordingly has received. Then the selected participant C decides on the back transfer by entering the corresponding amount. If a participant C has to make several decisions, they appear simultaneously on the screen in an arbitrary order.

General instructions for the participants

(Note: This is an English translation of the German instructions of the treatment CTP. We integrated control questions about the experiment into the z-tree file.)

Structure of the experiment

In this experiment you are always a group of three. In this triad there is always a **participant A**, a **participant B** and a **participant C**. At the beginning of the experiment the computer randomly determines if you are a participant A, B or C. You will keep the same role during the whole experiment.

The experiment lasts for ten rounds. We explain the procedure of one round. All ten rounds have the same procedure. You will be paid according to the points achieved in a randomly chosen round.

Participant A and participant B are endowed with 10 points. Participant A can send between 0 and 10 points to participant B. The amount sent is tripled by the experimenter and given to B. A **participant C** who has been selected by player B can now decide how many of the received points to return back to participant A. This back transfer is not tripled. C cannot return more than B received from A. The 10 points that B received from the experimenter remain with B in any case.

Participants A and B are randomly matched in each round. Participant B selects a participant C in each round. The selection procedure runs as follows: All participants C tell the participants B, what **percentage** of the transferred money they want to remain with B. At this stage each participant C gets a number, which clearly identifies her over all rounds without compromising her anonymity. Afterwards each participant B selects a participant C. Note that several participants B can simultaneously select a player C. The announcement of participant C concerning the back transfer is not binding. Thus, a selected participant C can reconsider her decision regarding the back transfer.

C's salary, which the experimenter pays, is independent from her own decision regarding the back transfers and depends only on the number of participants B selecting that participant C. If no participant B selects a specific participant C, this participant C receives 5 points. For every selection by a player B she gets additional 5 points. That means, if three participants B select a participant C in a round, she receives 20 points. The participants will receive the following payment, if the computer determines this round for the payment:

- Participant A: 10 points – amount sent by participant A + back transfer
- Participant B: 10 points + 3 × (amount sent by participant A) – back transfer
- Participant C: 5 points + 5 points * number of B's selecting that C

At the end of a round the participants will be informed about their points earned in that round.

Sequence of decisions:

A round proceeds on the screen as follows. Firstly, all participants C inform the participants B about the percentage of the transferred money that should remain with B. Secondly, each participant B selects a participant C. Simultaneously participant A decides on the transfer to B by entering a number between 0 and 10 and reports his belief concerning the expected back transfer.

The selected participant C then learns how many points A has sent and how many points B accordingly has received. Then the selected participant C decides on the back transfer by entering the corresponding amount. If a participant C has to make several decisions, they appear simultaneously on the screen in an arbitrary order.